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When Do Creditor Rights Work?

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Abstract

Creditor-friendly laws are generally associated with more credit to the private sector and deeper financial markets. But laws mean little if they are not upheld in the courts. The authors hypothesize that the effectiveness of creditor rights is strongly linked to the efficiency of contract enforcement. This hypothesis is tested using firm level data on 27 European countries in 2002 and 2005. The analysis finds that firms have more access to bank credit in countries with better creditor rights, but the association between creditor rights and bank credit is much weaker in countries with inefficient courts. Exploiting the panel dimension of the data and the fact that creditor rights change over time, the authors show that the effect of a change in creditor rights on change in bank credit increases with court enforcement. In particular, a unit increase in the creditor rights index will increase the share of bank loans in firm investment by 27 percent in a country at the 10th percentile of the enforcement time distribution (Lithuania). However, the increase will be only 7 percent in a country at the 80th percentile of this distribution (Kyrgyzstan). Legal protections of creditors and efficient courts are strong complements.

This paper—a product of the Enterprise Analysis Unit, Financial and Private Sector Vice Presidency—is part of a larger effort in the Bank to study the effects of regulation on access to finance. Copies of the paper are available free from the World Bank, 1818 H Street NW, Washington, DC 20433. Please contact Sushmitha Narsiah , room F4P-250, telephone 202-458-8768, fax 202-473-2029, email address snarsiah@worldbank.org. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The authors may be contacted at msafavian@worldbank.org or ssharma1@ifc.org. August 2007. (42 pages)

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1 Introduction

A large body of recent research on law and economics documents a positive association between legal institutions and financial development (La Porta et al. 1997, 1998; Levine 1999, Djankov et al. 2007). Scholars now generally concede that legal institutions matter to financial markets, and attention has turned towards identifying and quantifying the specific mechanisms which link law and finance.

We contribute to this debate by focussing on finance and two aspects of legal systems: the laws on creditor rights and the quality of contract enforcement by courts. We study how well laws on creditor rights work under varying conditions of court enforcement. Our main finding is that while strengthening creditor rights increases credit to firms, the payoffs from reforming these rights is lower in countries where the enforcement system functions poorly.

Financial markets are plagued by risk and informational asymmetries between borrowers and lenders, but collateral pledged by borrowers helps attenuate adverse selection and moral hazard in these markets (Stiglitz and Weiss 1981; Besanko and Thakor 1987). This is because collateral acts both as a signaling device by the borrower, and as an instrument ensuring good behavior of borrowers, given the existence of a credible threat to expropriate assets when contracts are violated (Aghion and Bolton 1992; La Porta et al. 1998). It follows that the extent to which the legal framework allows creditors to enforce their rights to collateral matters to the working of credit markets. This hypothesis has been broadly supported by a number of empirical studies. La Porta, Lopez-de-Silanes, Shleifer and Vishny (1997; henceforth LLSV) find that in a cross-section of 49 countries, a creditor rights index which captures the extent to which creditors can control the bankruptcy process is positively associated with the ratio of credit to GDP. Djankov, McLeish and Shleifer (2007; henceforth DMS) show that the same association holds in a sample of 129 countries during the period 1978-2003.

Within this field of inquiry into legal systems and finance, several cross-country studies (LLSV 1997; Beck et al. 2004; Qian and Strahan 2005) have shown that besides laws on the books, judicial enforcement is also related to financial outcomes. Laeven and Majnoni (2005), for instance, find that judicial efficiency is negatively correlated with interest rate spreads across countries. Johnson, McMillan

and Woodruff (2002) show that court enforcement of property rights is important to firm investment, and hence, to firms' demand for finance. Studies indicate that even within a country, financial outcomes vary across regions that have the same laws but different court efficiency. Bianco et al. (2005) find that in Italian provinces with longer trials or large backlogs of pending trials, credit is less widely available than elsewhere, while Marcella et al. (2001) show that Argentinean provinces with poor legal enforcement have less credit available to borrowers and more non-performing bank loans.[1]

Thus, there is mounting evidence that both laws and their enforcement matter in credit markets. However, while the literature acknowledges that the two are distinct aspects of the legal system, the empirical approach so far has mostly assumed that they work independently of each other. This assumption ignores the possibility that the effectiveness of laws depends directly on the institutions which are meant to implement them. For instance, the government may have limited resources to enforce new laws. The judiciary may not have the training to understand how these laws are applied in practice, or it may be slow in resolving credit contracts that are in dispute. Dishonest government officials could subvert the spirit of these laws, and in case credit contracts are in dispute, judges may be bribed to influence the outcome of a case.

It is well documented that the quality of courts as enforcement mechanisms can vary independently of the quality of the law on the books (LLSV 1998; Pistor 2000). Furthermore, there is evidence that across countries, the quality of laws does not substitute for the quality of enforcement (LLSV 1998).[2] If this is true- that laws and court enforcement are compliments rather than substitutes- then reforms to creditor rights will be more effective in settings where courts are more efficient.

We use firm survey data from East European and Central Asian countries to explicitly test the hypothesis that improving creditor rights in loan contracts is more effective when courts can enforce contracts by resolving disputes fairly and quickly. In our regression analysis of bank credit used by firms, we implement this test by estimating the coefficient on the interaction between a creditor rights index and measures of court efficiency.

Our methodology is based on the fact that laws on the books and legal enforcement are conceptually and empirically distinct. The distinction between rights and enforcement is particularly salient in our

sample of transition economies, given the widespread evidence on the "transplantation" of new laws into Eastern Europe after the fall of communism (Fairgrieve 1998; Dahan 2000; Pistor et al. 2000).

Collateral (secured transactions) laws were transplanted during the transition because readily exportable models were available to recipient countries, and because these laws were considered a high priority for reform that sought to catch up with the west and meet EU regulations. Indeed, many of these laws were adopted rapidly in the region, heedless of the legal and institutional context in which they were meant to function. The result, as reported in Pistor et al. (2000), was that the formal investor protection achieved in CIS countries by 1998 was not mirrored in measures of legal effectiveness.[3] This evidence on the rapid borrowing of laws is also supported by the absence of a positive correlation between changes in creditor rights and enforcement measures in our data.

In addition to our focus on the interaction of law and legal effectiveness, our use of firm-level data also distinguishes us from most of the previous work on law and finance. Cross-country analysis which looks at country level financial aggregates can at best only suggest at the mechanisms affecting the observed aggregates, and cannot examine if different types of borrowers and lenders are affected differently. In recognition of these shortcomings, there is now a trend towards using micro level data. Thus, Visaria (2006) looks at bank loans to measure the microeconomic effect of debt-recovery tribunals in India, and finds that their establishment reduced delinquency in loan repayment. Haselmann et al. (2006) show that in their sample of bank loans from 12 transition countries, lending increased in response to changes in collateral and bankruptcy laws, and to a greater extent for newer banks. Our study adds to this picture by using data from firms instead of banks, and by showing that the effect of creditor rights reforms varied not only under different measures of enforcement, but also across different types of firms.

Focussing on the interaction of creditor rights and enforcement also allows us to address the endogeneity problem that other studies have faced, namely, the possible correlation between the levels of these variables and other unobserved determinants of bank lending. We argue that such omitted variables are unlikely to be correlated with the interaction of rights and enforcement. What is more, the interaction of enforcement with *changes* in rights is even less likely to be correlated with changes

in unobservable determinants of credit. Thus, exploiting the panel dimension of our data and the fact that creditor rights change over time, we show our result to be robust to the inclusion of country and firm fixed effects.

Our results also survive a number of sensitivity tests. They are robust to using different national or sub-national measures of enforcement capacity, and to allowing time trends in credit to have varied across industries or across regions with different enforcement. We show that they are not affected by allowing the effect of creditor rights to differ across different sized firms or across rich and poor countries. Addressing the concern that the findings are specific to our sample of transition economies, we are able to replicate them on a larger cross-country data set which has information on creditor rights, court enforcement and the private credit to GDP ratio for 129 countries over a period of 25 years.[4]

Our main result indicates that the effect of creditor rights reform on bank credit depends on court efficiency, and that these reforms have significantly higher payoffs when courts have the capacity to enforce contracts. The point estimates imply that reforming creditor rights will have a low positive impact in a country at the lower end of the enforcement distribution, but will be remarkably effective in high enforcement countries. In particular, the point estimate of the differential impact of creditor rights implies that a unit increase in the creditor rights index will increase the share of bank financing in firm investment by 27 percent in a country at the 10th percentile of the enforcement time distribution (Lithuania). However, the increase will be only 7 percent in a country at the 80th percentile of this distribution (Kyrgyzstan). This finding has important implications for the need for judicial reform, and for how the policy choice between different financial market reforms may depend on the underlying enforcement institutions.

The rest of the paper is organized as follows. The next section describes our data, and presents summary statistics and correlations among the main variables. Section 3 spells out our empirical strategy, including the robustness analysis. Section 4 presents the estimation results, and Section 5 concludes.

2 Data and Main Variables

2.1 Firm Level Data

We test our hypothesis about the joint effect of creditor rights and enforcement on bank credit using firm data collected through the World Bank's "Enterprise Surveys" conducted in 2005 and 2002.[5] For the year 2005, we have data on 10,500 firms from 27 countries situated mainly in Eastern and Central Europe. We use these data in our cross-section analysis. The 2002 surveys covered fewer countries, but a subset of firms surveyed in 2002 were resurveyed in 2005. We use this data set, which includes 1200 firms observed in 2002 and 2005 across 21 countries, to estimate panel regressions with firm fixed effects.

As a robustness check, we also test our hypothesis on the cross-country data set used by Djankov et al. (2007) (hereafter, DMS). This data set covers 129 countries over the period 1978-2005.

Below, we describe the main variables used in this study. In addition, Table 1 gives a detailed description of these main variables, and Table 2 summarizes them by country.

2.2 Dependent Variables

Our main outcome variables are measures of the extent to which banks finance firms. Specifically, these variables are the percentage of a firm's new investment that is financed through bank loans, and a binary indicator for whether a firm has a bank loan outstanding. These variables are based on separate questions in the firm surveys. The bank financing share is our best firm-level measure of overall levels of bank credit. However, unlike the binary loan indicator, it cannot distinguish between scenarios where more firms have bank loans, or the same set of firms have larger loans. Thus, the loan dummy enables us to look at the extensive margin of change in bank lending, and also to check that our results are not driven by large changes in credit for a few firms.

In regressions using the country-level DMS data set, our dependent variable is private credit relative to GDP. This variable measures total credit to the private sector from the banking sector.

2.3 Explanatory Variables

Our main explanatory variables are measures of creditor rights (CR), credit information (CI) systems and court enforcement in the surveyed countries.

We measure the legal rights of creditors using the Creditor Rights Index developed in DMS. This index measures four powers of secured lenders in bankruptcy, with higher values indicating stronger creditor rights over collateral. Table 1 describes the four components of the CR index. Whenever possible, we also use a newer, finer version of this index, which ranges in value from 1 to 9 and is updated annually in the World Bank's Doing Business Report.[6]

Besides creditor rights, the literature on financial market reforms has also identified credit information systems as a mechanism for improving financial intermediation. Therefore, we consider it important to control for CI levels in our analysis. We measure CI by a binary indicator for whether a public credit registry or a private credit bureau exists in the country. This indicator too was used in DMS, and has since been updated in the Doing Business reports.[7]

Contract enforcement is a broad concept which cannot be captured in a single index. The strategy we adopt is to use several alternative measures of enforcement; these vary at either the national or the sub-national level. At the country level, we use *CourtTime*, a measure of the time in calendar days to resolve a contract dispute. Higher values of this variable imply poorer court enforcement. The methodology behind this measure was developed in Djankov et al. (2003). A comparable measure of time in court, which is based on this methodology, is updated annually in the Doing Business Report.

The variables *CourtSpeed* and *Enforce* are similar measures of enforcement, but based on firm responses to questions about judicial efficiency in our survey data. Firms were asked to rate a number of key dimensions of court efficiency on a scale of 0 to 6, with higher rankings associated with more efficient courts. *CourtSpeed* is a measure of the firm's perception of the speed of judicial enforcement, with higher values implying faster courts. *Enforce* is a measure of the firm's confidence in judicial enforcement, with higher values implying that courts are more capable of enforcing decisions.

Firm responses to questions about judicial efficiency may reflect firm characteristics and perfor-

mance, and so might be correlated with unobserved determinants of the supply and demand for credit. To account for this possible endogeneity in firm perceptions of court enforcement, we average these firm responses by regions within countries. Thus, *CourtSpeed* and *Enforce* vary at the sub-national level.[8]

On average, across all the countries in our data, the percentage of firm investment financed by banks is about 11%. However, as the cross-section data summary statistics presented in Table 2 show, there is considerable variation in bank financing across countries. For instance, in Georgia, this figure is 23%, while in Turkey, it is about 6%. There is also significant variation in creditor rights and court enforcement across the 27 surveyed countries. The variable *CourtTime* ranges from 980 days in Poland to 150 days in Estonia. Similarly, the average of the firm rating of court enforcement is 4 in Estonia, while it is 3 in Poland. Table 3 presents changes in the creditor rights index between 2002 and 2005. During this period, these rights improved in four countries- Bulgaria, Hungary, Poland and Russia- and worsened in one country- Romania.

2.4 Correlations

Table 4 presents correlations between the levels of the main explanatory variables in 2005. Creditor rights, credit information bureaus and national income are all positively and significantly correlated with the percentage of firm investment which is financed by bank loans. Time taken by courts is negatively correlated with bank financing, indicating that in countries with slower courts, firms on average have lower bank financing of new investment. In general, there is a positive correlation between all three measures of enforcement, creditor rights and national income.[9] These correlations imply that in a cross-section, it is difficult to identify separately the financial market impacts of creditor rights, enforcement and general economic development.

Table 5 presents correlations between the three measures of the level of court enforcement and the changes, in our 2002-05 panel data, in creditor rights and credit information bureaus. Increases in creditor rights were negatively correlated with enforcement, although the correlations are weaker for the firm-reported measures of enforcement. If our hypothesis is correct, then this correlation implies that regressing change in creditor rights on change in bank credit without interacting the former with

enforcement would lead to an underestimation of the effect of rights. We also note that creditor rights increases were positively correlated with national income, while the correlation was negative for credit information increase. This is consistent with the observation in DMS that rich countries are more likely to choose creditor rights over credit information systems.

2.5 Illustrations of the Empirical Approach

Our empirical strategy allows us to deal with the problem presented by the correlation in rights, enforcement and income by focussing on the interaction of creditor rights and enforcement. Before moving to the regression specification, we give two graphical illustrations of the strategy.

In the cross-section, our focus on the interaction between creditor rights and enforcement is a double difference approach, since it compares how the effect of a difference in rights differs across good and bad enforcement. Figure 1 depicts a simple illustration of this approach. The x-axis measures creditor rights and the y-axis, *CourtTime*. The boldface number in every quadrant is the average, across all firms in all countries that fall in the quadrant, of the percentage of new investment financed by banks. As we move right along the x-axis, that is, from countries with weak creditor rights to those with strong rights, the increase in bank financing is higher in the lower half of the graph, which is where countries with better enforcement lie. This shows that in our data, the association between rights and bank finance is stronger where enforcement is better.

In the panel, our strategy is a triple difference approach, since it looks at how the effect of a change in rights differs across good and bad enforcement. Likewise, Figure 2 depicts the association, in our panel data, between increases in creditor rights and increases in bank financing. These are shown separately for countries with slow courts and those with fast courts. As expected, the relationship is more positive when courts are efficient.[10]

3 Empirical Strategy

To test how creditor rights, credit information systems and court enforcement affect bank credit, we regress firm credit outcomes on measures of these institutional variables. Since we are interested in how the effect of creditor rights depends on enforcement, we allow the coefficient on creditor rights to vary by enforcement. Our basic regression specification is:

$$x_{ijt} = \alpha + \beta CR_{jt} + \gamma CI_{jt} + \nu Enforce_j + \beta_2 CR_{jt} * Enforce_j + \epsilon_{ijt} \quad (1)$$

Here, CR_{jt} and CI_{jt} measure, respectively, creditor rights and credit information in country j in period t. $Enforce_j$ measures contract enforcement capacity in country j. This variable is assumed to be fixed over the time span studied. The main coefficient of interest is β_2, which is the coefficient on the interaction of creditor rights with enforcement. It measures how the relationship between creditor rights and the outcome variable varies with contract enforcement capacity. If enforcement is increasing in $Enforce_j$, then a positive β_2 indicates that the impact of creditor rights on bank credit is increasing in court enforcement. This is what our hypothesis leads us to expect.

We first estimate the above equation on the cross-section of 10,500 firms that were sampled by the World Bank in 2005. This regression allows us to look at the association between bank credit, credit institutions and contract enforcement capacity at the firm level, and across countries. This improves on past studies which were restricted to country-level credit outcomes, because such studies could not examine how many firms and which sort of firms are affected by credit law reforms. Since most of the variation in the explanatory variables is at the country level, we always present robust standard errors which are clustered by country.

We estimate equation (1) using several alternative measures of enforcement. For every enforcement measure, we focus on two firm-level bank credit outcomes. The first outcome variable is the proportion of new firm investment that is financed by bank loans, and it allows us to study how the average quantity of bank lending changes across firms. This measure reflects both changes in the set of firms that get bank loans, and changes in the average amount borrowed. In order to delineate the first of these effects,

we use as our second outcome variable a binary indicator for the firm having a bank loan. It allows us to see if the set of firms that get access to any bank credit changes.

The assumption which enables us to identify the interaction effect in a cross-section is that the interaction of creditor rights and enforcement is uncorrelated with unobserved determinants of bank credit. In a sample of fewer than thirty countries in which most of these variables are measured at the country level, it is possible that this assumption is violated. For instance, creditor rights, court enforcement and gross domestic product tend to be positively correlated across countries, and it could be that the interaction term picks up some non-linear effect of overall wealth or development on bank credit. Moreover, firms could be systematically different across countries that vary in the interaction of credit institutions with enforcement.

To deal with these issues, we first add firm and country level controls to the cross-section estimations . In particular, we add controls for firm size, ownership and industry. We control for differences in the overall level of development with the logarithm of gross national income. Adding these controls does not affect the estimate of β_2.

Our second strategy for dealing with the endogeneity problem exploits the fact that a subset of the 2005 sample consists of firms that were visited in 2002. Since creditor rights change over time, we can identify how the effect of a change in CR varies by enforcement. We do this by estimating the above equation on the panel of firms, and allowing for firm specific fixed effects:

$$x_{ijt} = \alpha_{ijt} + \beta CR_{jt} + \gamma CI_{jt} + \beta_2 CR_{jt} * Enforce_j + \nu Year_t + \epsilon_{ijt} \tag{2}$$

The firm fixed effect α_{ijt} controls for all unobserved country or firm level characteristics that are correlated with the explanatory variables. Thus, the differential impact of creditor rights on bank credit is identified by seeing how the relationship between changes in CR and changes in firms' bank credit usage varies by enforcement. The identifying assumption in this specification is that *changes* in unobserved determinants of bank credit are uncorrelated with the interaction of changes in creditor rights *and* enforcement. Note that this assumption holds even if changes in unobserved determinants

of bank credit are correlated with court enforcement, or with creditor rights reforms alone.

3.1 Robustness Analysis

One concern with the regression specification described in equation 2 is that it does not allow the effect of changes in credit information to vary by court enforcement. The results in DMS indicate that information and creditor rights are alternative ways to improve financial markets, and that credit registries seem to matter more in poorer economies. Since poorer economies tend to have worse court enforcement, this finding suggests that enforcement is complementary to creditor rights but not to credit information systems.

Nonetheless, it is safer to assume that both creditor rights and credit information bureaus could have a heterogenous effect depending on enforcement. We prefer this more general empirical specification because there is a negative correlation between changes in creditor rights and information in our data. Therefore, all the panel estimation results presented are those in which both creditor rights and credit information are interacted with enforcement.[11]

Except for one case (Romania), creditor rights either stayed constant or increased over time in our panel. As a result, any unobserved shocks to financial markets during this period that also varied systematically across countries with different courts would be picked up by the interaction of creditor rights with courts. The most flexible control for such heterogenous shocks is to allow for a differential time trend by enforcement, [12] and we show that our results are not sensitive to allowing countries which differ in enforcement to have different time trends in bank credit.[13]

The second concern in estimating equation 2 is that court enforcement is positively correlated with more general measures of development, such as national income. This would be an issue in interpreting our results if the effect of creditor rights depended on other dimensions of institutional quality which are correlated with, but not the same, as court enforcement. It would not be a problem if such institutions mattered to creditor rights only through their affect on contract enforcement. A related concern is that the extent of the market failure which creditor rights address might be correlated with enforcement across countries. If so, enforcement would also proxy for the extent of financial market failure. However,

note that creditor rights will have a greater impact where risk and informational asymmetries are more pronounced. Since these market failures are likely to be more acute in less-developed countries, this bias would be in a direction opposite to the interaction effect our hypothesis implies, and so would not drive our results.

Nevertheless, we cannot rule out the possibility that the effect of creditor rights depends on other institutions that are correlated with the overall level of development but have nothing to do with enforcement. We address this issue by estimating a modified version of equation 2 in which we also allow for a differential effect of creditor rights across rich and poor countries. This is done by including an interaction between national income and creditor rights as a control variable.

Following the observation in Pistor et al. (2000) that transition economies were able to transplant laws but not the institutions that enforce them, we consider enforcement to be a "initial" institutional condition in this study. In other words, we are interested in the long run component of court enforcement. Our measure of this long-run institution - court efficiency measured in 2002- is imperfect if there were substantive changes in enforcement procedures during this period. This could bias our estimates of the interaction, but only if changes in court efficiency were correlated with initial court efficiency.

However, neither concern appears to be the case in our data. Our court efficiency measures do not change much during 2003-05. For example, the Doing Business measure of court time changes in 7 of the 21 panel countries, and the median change in these 7 countries is only -6%. Moreover the change, if any, is uncorrelated with 2003 levels of court efficiency. The correlation between the change in the Doing Business measure of court time and court time in 2003 is -0.14, and it is not significant at the 10% level. As a result, country rankings by court efficiency stay the same during this period. Thus, our preferred specification uses a fixed court enforcement measures. We also show that our panel results are unaffected if we replace fixed enforcement measures with contemporaneous, time-varying measures.

Another concern with our analysis is that the identification relies on changes in creditor rights across a relatively small number of countries over a short period of three years. Our identification strategy, which is based on a triple differencing, insulates us from the usual endogeneity problems seen in most cross-country studies. But it is possible that our results are driven by some unobserved development in

a few of the sampled countries. We have verified that our results are robust to dropping outliers in court enforcement. As a further check, we replicate our estimation results on a data set which comprises 129 countries over a twenty-five year period. This is the country-level panel data set which is used in DMS. Specifically, we run the equivalent of our main specification on this data set by putting in country fixed effects, and estimating the coefficient on the interaction between time-varying creditor rights and the DMS measure of court time.

It is possible that changes in creditor rights have a heterogeneous effect on firms, and depend on firms characteristics like size and ownership. There is evidence in the literature that creditor rights have a heterogeneous effect across banks (Haselmann et al. 2006), and the same could be true of firms. Moreover, if the distribution of these firm characteristics varied systematically across countries that differ in enforcement, then this heterogeneous impact could bias our estimate of the interaction effect of laws and enforcement. We check that this is not the case by adding interactions of creditor rights with firm size and ownership variables as controls, and finding that this does not change our estimate of the coefficient on the interaction between law and enforcement.

4 Results

4.1 Cross-section Regression Results

Tables 6 and 7 present the results from estimating equation 1 by OLS on the cross-section of firms surveyed by the World Bank in 2005. In Table 6 the dependent variable is the percentage of new investment financed by bank loans. The first pair of regressions use time in court (*CourtTime*) to measure enforcement. The second pair measure enforcement by regional averages of firms' perception of judicial enforcement (*Enforce*), while the third pair use regional averages of firms' perception of the speed of courts (*CourtSpeed*). We find that for all these measures, the sign of the estimated coefficient on the interaction of creditor rights with enforcement goes in the direction our hypothesis leads us to expect. It is significant for both firm-reported measures of enforcement (columns 3-6), though not so for the country-level measure *CourtTime* (columns 1-2).

As reported in column (3), the coefficient on the interaction of creditor rights with *Enforce* is 0.305, significant at the 1% level. Since *Enforce* is an index increasing from 1 to 6, this estimate implies that the impact of a unit change in creditor rights on the fraction of new investment financed by banks increases by 1.5 percentage points as enforcement improves from the best to the worst possible. The estimated interaction effect is similar in Column 4, which presents the same regression but with additional controls, namely firm size, industry, ownership, and the logarithm of gross national income. Columns 5 and 6 show that this pattern is repeated when we measure enforcement by *CourtSpeed*. Since *CourtSpeed* also ranges in value from 1 to 6, it is good to note that the point estimates of the interaction effect are of similar magnitudes to those in column 3.

Table 7 presents the same set of regressions; here the dependent variable is an indicator for whether the firm has a bank loan. This variable is insensitive to the amounts borrowed, and measures the extensive margin of change in bank lending. We find that the results for this outcome resemble those in Table 6: the interaction of creditor rights with enforcement is positive and significant for both firm-reported measures of enforcement (columns 3-6).

In a cross-section of countries, it is possible that the interaction of credit laws and court enforcement is correlated with unobserved differences in country characteristics which affect financial outcomes. Moreover, we are unable to assign an expected sign to this omitted variable bias. These country "fixed effects" could be why the country-level measure of time in courts does not give significant results. In the next section, when we control for this bias by exploiting changes in creditor rights, we find that all three measures of enforcement give consistent and significant results.

4.2 Panel Regression Results

The panel dimension of our data allows us to look at how changes in a firm's use of bank credit are related to changes in credit institutions, and how this relationship varies with court enforcement. Table 8 estimates equation 2 with the country-level measure *CourtTime* interacted with creditor rights and credit information. In column (1), the dependent variable is the share of bank credit in new investment. The coefficient of interest, the interaction of creditor rights (CR) with *CourtTime* is negative (-0.007)

and significant at the 1% level. Since higher values of *CourtTime* mean slower courts, the negative estimate indicates that the effect of creditor rights on the use of bank credit for new investment by the average firm is increasing in the speed with which courts resolve disputes. This is in keeping with our hypothesis that reform works better when courts can enforce the new laws.

Suppose that the time taken by courts to resolve a dispute decreases by 100 days, a change well within the range of our data on court time. Then, the point estimate of the interaction term implies that the impact of a unit increase in creditor rights on the percentage of new investment financed by bank loans would rise by 7 percentage points. Given that in our sample, investment financed by bank loans is 11% on average, this 7 percentage point differential is of remarkable magnitude.

To check against the possibility that this result is driven by a positive shock to bank credit in countries with faster courts, in column (2) we allow the time trend to vary by *CourtTime*. This does not affect the sign or the significance of the creditor rights and court time interaction coefficient. In fact, the estimated magnitude of the interaction effect is now greater. Thus, our result is robust to allowing the change in bank credit to have differed by enforcement.

In Columns (3) and (4) we report regressions in which we have used the dummy for a bank loan outstanding as the dependent variable. The interaction term between *CR* and *CourtTime* is, once again, negative and significant. This indicates that the effect of credit law reform on the set of firms that get a bank loan is increasing in the speed of court enforcement.

The next pair of tables present regressions similar to those reported in Table 8, but using the firm-reported measures of judicial efficiency. As in the previous table, for each enforcement measure, we look at two outcomes: the share of new investment financed by bank loans, and the binary loan indicator. Table 9 uses firm reported level of judicial enforcement (*Enforce*), while Table 10 uses the firms' rating of the speed of courts in resolving disputes (*CourtSpeed*). For both measures, higher values indicate better enforcement capacity.

We have averaged these firm-reported measures by region within every country. This is to account for the potential endogeneity of firm perception. Firms in the same area may report different experiences with the judiciary not just because they faced different types of courts but also because they had

different characteristics or were in different situations. These unobserved firm characteristics would be a problem if correlated with credit usage. Taking regional averages avoids this bias, and at the same time exploits within-country variation in court enforcement.

In Table 9, the estimate of the coefficient on the interaction between creditor rights and *Enforce* is positive and significant for both measures of bank credit. Thus, the impact of creditor rights is increasing in regional judicial enforcement. The same pattern of positive and significant interaction terms (*CR*CourtSpeed*) for both outcomes, and both with and without differential time effects, is seen in Table 10. Thus, these panel regression results strongly suggest that the effect of a reform in creditor rights on bank lending varies significantly with the enforcement capacity of courts.

4.3 Robustness Checks

Table 11 presents results from estimating equation 2 after adding the interaction of creditor rights with the logarithm of national income as a control variable.[14] As explained in section 3.1, this is to test for the possibility that our results are driven by a differential impact of creditor rights across rich and poor countries, instead of across good and bad courts. We present results for all three measures of court enforcement. Columns 1-3 show that in none of the cases is our estimate of the *CR* and enforcement interaction affected by allowing creditor rights to have a differential impact by national income. This indicates that our results are not driven by the correlation of court enforcement with more general indicators of development.

Our measure of long-run enforcement - court efficiency in 2003- is imperfect to the extent that there were substantive changes in enforcement procedures during this period. Table 12 replaces these fixed measures with contemporaneous time-varying measures of court efficiency to show that our results are robust to allowing enforcement to change over the sample period. Columns (1) and (3) use time-varying *CourtTime*, while (2) and (4) use time-varying *Enforce*. As in the previous tables, we look at two outcomes for each enforcement measure: the share of new investment financed by bank loans (columns 1-2), and the binary loan indicator (columns 3-4). In each specification, the interaction of *CR* with time-varying enforcement is significant and of the same sign and magnitude as in the corresponding

estimation using fixed enforcement. For example, in column (1), the coefficient on *CR*CourtTime* is -0.008, significant at 1% level. This is comparable to the *CR*CourtTime* estimate of -0.007 in column (1) of Table 8, which corresponds to the same specification with fixed *CourtTime*. Likewise, in column (2) of Table 12 the estimate of the coefficient on *CR*Enforce* is 5.97; the corresponding estimate for the interaction of *CR* with fixed *Enforce* was 6.69 (column (1) of Table 9).

Next, in Table 13 we address the concern that our results could be sensitive to unobserved changes in a few countries that also happen to be correlated with the interaction of credit law reform and enforcement. We do so by estimating the equivalent of equations 1 and 2 on a panel of 129 countries over the twenty-five year period 1978-2003. This is the data set which was used in Djankov et al. (2007). As the authors note, there were about 30 instances of credit law changes during this period. This data set has the same creditor rights and credit information index that we have used in all our estimations; moreover, their measure of contract enforcement time is identical to *CourtTime*. The outcome variable in these country-level regressions is the ratio of private credit to GDP.

As in all the previous regressions, we test our hypothesis by interacting creditor rights with enforcement time. Columns 2 and 3 in Table 13, which include country fixed effects and so are the equivalent of our firm fixed effects estimates, show that our results are replicated on this wider and deeper panel. Column 3 shows that this is robust to allowing the time trend to differ by enforcement. Once we control for the different scale of the firm-level and the country-level outcome variables, even the magnitude of the estimated interaction term is close to our firm panel estimate.

4.4 What Do Our Estimates Imply?

As measured by time spent in enforcing a contract through courts, there is substantial variation in contract enforcement across countries. Our estimates of the interaction term allow us to measure the effect of this variation on the impact of creditor rights on the percentage of firm investment financed by banks.

Consider a country close to the 10th percentile of the distribution of *CourtTime* across the 129 countries in the DMS data. In Lithuania, it takes only 153 days to resolve contract disputes in courts.

According to the estimates in column 1 of Table 8, in Lithuania the effect of a unit increase in the creditor rights index would be 3.2 ($= 4.306 - 0.007 * 153$) percentage points.

Now consider Kyrgyzstan, which is at the 80th percentile of the distribution of enforcement time, with a *CourtTime* of about 500 days. In Kyrgyzstan, the effect of a unit increase in the creditor rights index would be only 0.8 ($= 4.306 - 0.007 * 500$) percentage points.

Suppose that as a result of reforms, the creditor rights index increased from 0 to 4 in both countries. Then, the numbers derived above imply that the share of new investment financed by banks would go up 12 percentage points in Lithuania. Since the average share of bank loans in firm investment was 11% in 2005, this is an increase of more than 100%. However, this share would increase by only 3.2 points, or 28%, in Kyrgyzstan. Thus, our results indicate that if court enforcement is good (within the observed bounds), creditor rights reforms have a remarkable impact on bank lending. But if court enforcement is poor, these reforms have a much smaller, though not insignificant impact.[15]

4.5 Heterogenous Effects of Creditor Rights and Enforcement

In Table 14 we examine if changes in creditor rights have a heterogeneous effect depending on firm characteristics like size and ownership. First, in column (1) we add firm size[16] interacted with *CR* as a control, and find that the estimate of the coefficient on *CR*CourtTime* is unaffected, being close to its value of -0.007 in column (1) of Table 8. In column (2), we then interact firm size with *CR*CourtTime*. This coefficient is estimated to be negative and significant, implying that the *CR*CourtTime* interaction effect was felt more strongly in larger firms. One explanation could be that larger firms have more tangible assets to put up as collateral, and so banks increase lending disproportionately to larger firms when there is a *de facto* reform in creditor rights.

Next, in columns (3) and (4), we allow the effect of *CR* and *CR*CourtTime* to vary by firm ownership. The variable *Govt.* is a dummy indicating majority government ownership of firm, while *Foreign* is a dummy indicating majority foreign ownership of firm. Thus, the omitted ownership category is domestic private ownership. Once again, the coefficient on *CR*CourtTime* is substantively unaffected by these controls. As column (4) reports, we also find that both *Foreign*CR*CourtTime* and

*Govt.*CR*CourtTime* have positive coefficients. This indicates that the *CR*CourtTime* interaction effect was strongest in domestic private firms. This may be because firms with foreign ownership can more easily access international capital markets. Compared to foreign-owned firms, domestic firms are more dependent on the financial sector environment in the country in which they reside. Therefore, we would expect the effect of any domestic or local reform to have a higher impact on domestic firms. This could explain why a *de facto* reform in creditor rights has a larger impact on private domestic firms.

5 Conclusion

For legal rights to have any meaning, disputes involving these rights need to settled quickly and fairly by courts. In the absence of enforcement, there is little to deter violations. Knowing this, banks hesitate to enter into contracts based on these rights. We show this to be true in the case of laws which give lenders better rights to collateral. In countries and regions with very poor court enforcement, reforms in creditor rights have relatively little impact on bank lending to firms. Such reforms, however, have a remarkable effect on bank lending where court enforcement is efficient.

The immediate implications of the measured interaction effect are that court enforcement matters in financial markets, and that it needs to be taken into account when prescribing reforms in collateral laws to improve access to credit. This is underscored by the fact that cross-country indicators of enforcement quality, such as the Doing Business measure of court speed in resolving disputes, show considerable variation across countries.

There is evidence that judicial enforcement varies significantly even within countries.[17] If this is the case, then creditor rights reforms may be effective only to the extent that regional and local governments can enforce the new laws, and the pay-offs from the reforms will vary accordingly across sub-national regions. Thus, reforms to creditor rights at the national level might increase regional inequality.

However, our estimates also indicate that even in countries such as Kyrgyzstan, which are near the lower end of the distribution of court enforcement, creditor rights reforms will have a small but appreciably positive impact on bank credit. This suggests that the mechanisms through which creditor

rights affect financial markets are partly independent of enforcement.

Thus, our findings have important policy implications about the design of creditor rights and other financial market reforms. Making creditor rights effective depends critically on enforcement, but improvements to the judicial enforcement system are likely to take time. This makes it worthwhile to consider reforming creditor rights in a manner that de-links them from court procedures. This can be done by introducing out-of-court enforcement proceedings, or summary proceedings for courts which reduce judicial discretion in the application of laws.

Notes

[1] Penheiro and Cabral (2001) find similar patterns in judicial efficiency and credit markets within Brazil. Laeven and Woodruff (2006) find that Mexican states with more effective legal systems have larger firms.

[2] "An investor in a French-civil-law country is poorly protected by both the laws and the system that enforces them. The converse is true for an investor in a common-law country, on average."(LLSV 1998).

[3] Pistor et al. 2000 find no positive correlation between either the level of laws on the books in 1998 or the change in the laws over 1992-98 and various measures of legal effectiveness in CIS countries.

[4]It is possible that both creditor rights and enforcement are driven by the third unobservable which also affects bank lending. For example, it might be that a CR reform in countries with good courts are driven by political economy considerations different from those driving a CR reform in a country with bad courts, and that such underlying causes also affect financial markets directly. We cannot think of a particular mechanism of this type; nonetheless, our results must be interpreted with this caveat in mind.

[5]See www.enterprisesurveys.org for a description of the survey methodology.

[6] See www.doingbusiness.org.

[7] Our cross-section estimations for 2005 use the newer Doing Business version of the CR and CI indices, since they are finer and contain more information. However, because the later versions of these indices are not available for 2002, the panel regressions use the original DMS indices.

[8]For the purpose of this study, we consider enforcement to be an initial institutional condition. Thus, the panel regressions use the level of *CourtSpeed* and *Enforce* in 2002. Since the earliest Doing Business report dates to 2003, we use 2003 *CourtTime* when employing the Doing Business enforcement measure. Sections 3.1 discusses this assumption in detail.

[9]Note that *CourtTime* is decreasing in better enforcement.

[10] The line depicted is a non-parametric "locally-weighted least square regression" estimate. This smoothing technique is used for illustration only, and the negative slope of this line in the slow courts panel should not be understood to imply that the relationship between creditor rights and bank financing is negative in countries with slow courts.

[11]None of the reported panel estimates of the interaction of enforcement with creditor rights are sensitive to including or dropping the interaction of credit information with enforcement.

[12]Since there are only two time periods in the panel, this is equivalent to interacting time dummies with enforcement.

[13]It is possible that growth opportunities differed across industries in our sample period. If so, and if the distribution of industries varied systematically across countries and was correlated with changes in creditor rights, there could be a correlation between changes in creditor rights and changes in growth opportunities. To control for this, we modified the main specification by including different time trends for different industries as controls. This did not affect the estimated interaction effect.

[14] Gross National Income averaged over 2001-2003. All results are robust to using gross national income instead of its logarithm, and to including the interaction of credit information with enforcement.

[15]The impact is almost nil at the very extreme of the court time distribution. For example, the coefficient on creditor rights would be zero if court time were 650 days.

[16]Measured by the number of permanent employees.

[17] See, for example, Laeven and Woodruff (2006) on Mexico.

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Figure 1: Bank Financing vs Creditor Rights and Court Time in the Cross-section Data

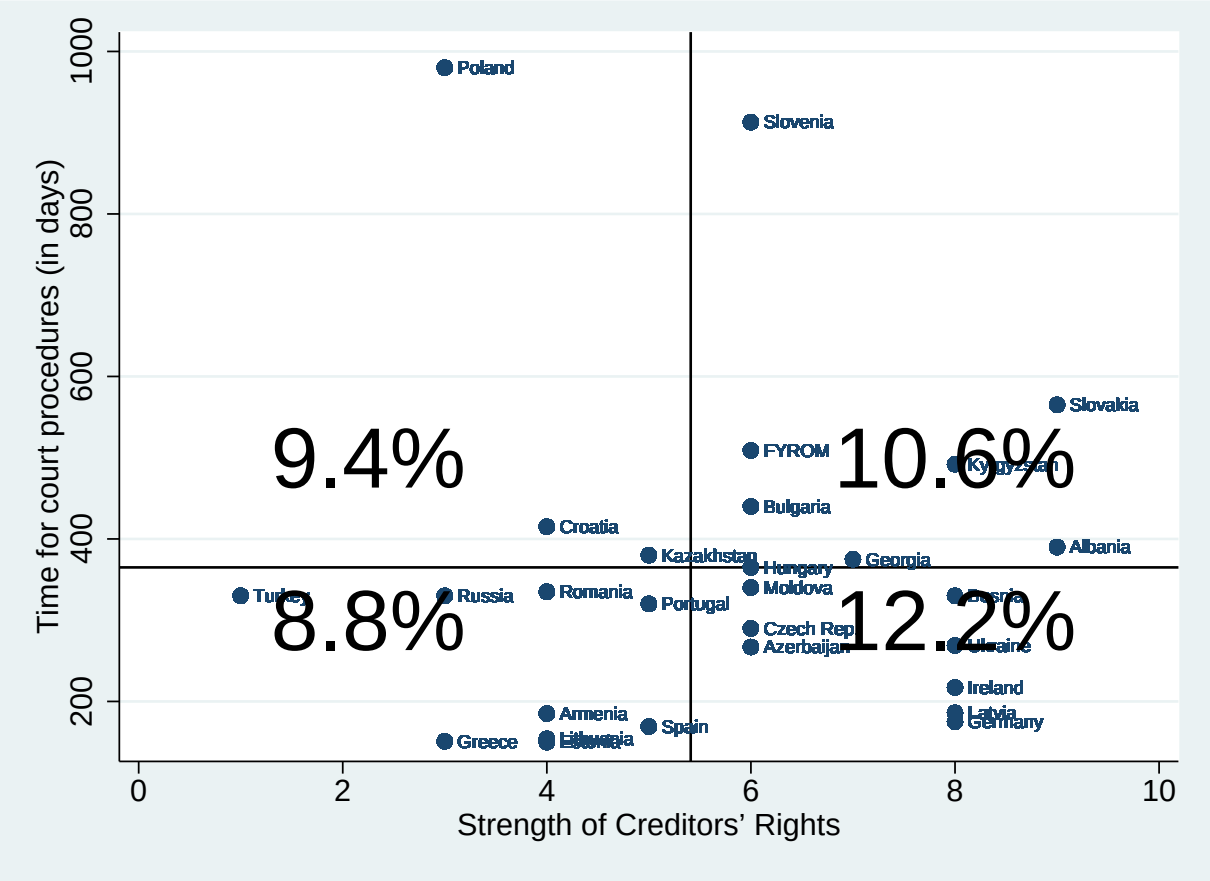

Figure 2: Comparing Increases in Creditor Rights vs Increases in Bank Financing Across Fast and Slow Courts

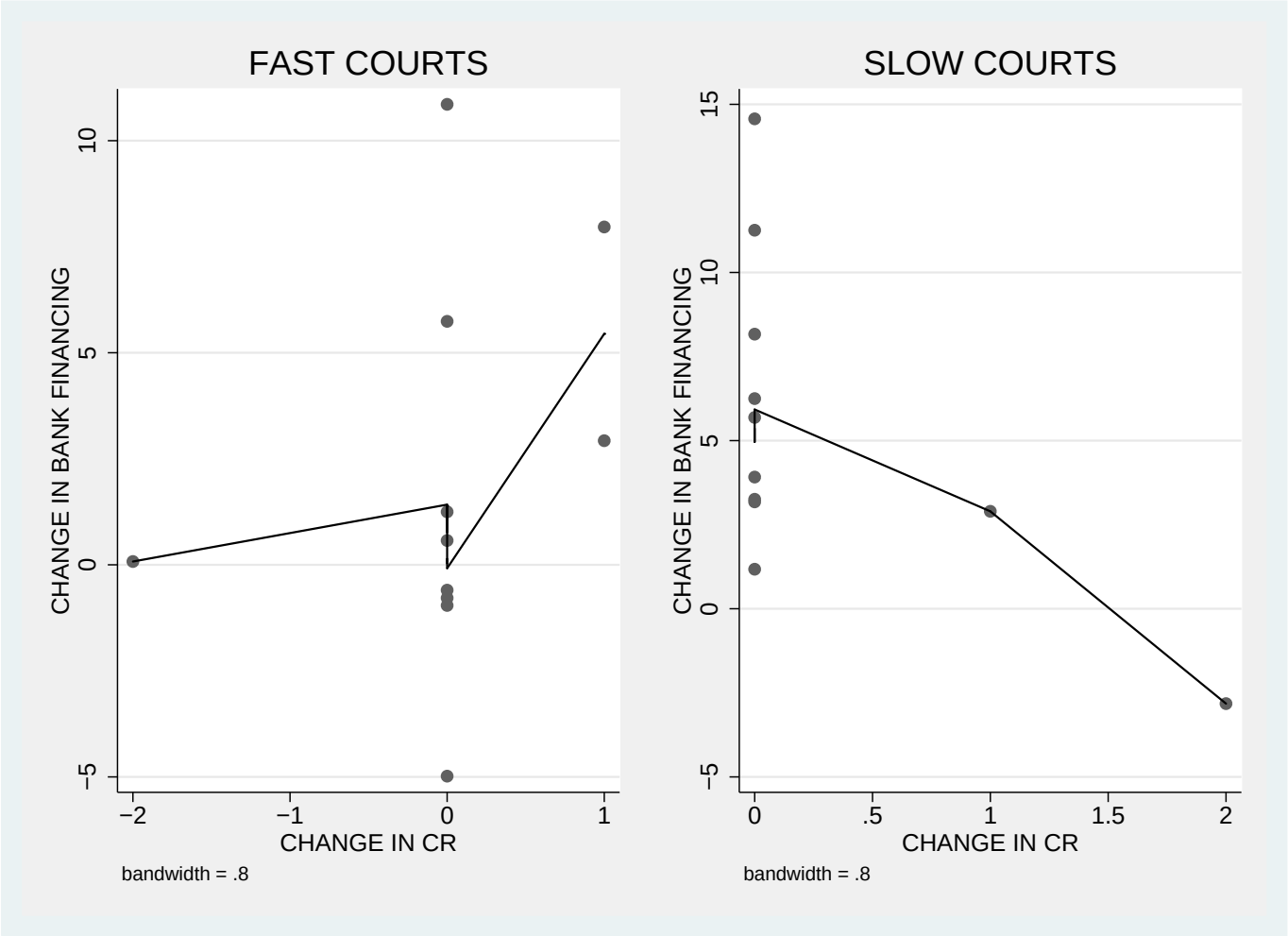

Table 1: Description of the Variables

Variable	Description
Bank Investment Finance	Proportion of firms' new investment financed by private, commercial banks. Source: World Bank Enterprise Surveys. Available at *www.enterprisesurveys.org*.
Bank loan	Binary variable indicating whether or not a firm has a bank loan outstanding. Variable takes on a value of 1 for a bank loan, and 0 otherwise. Source: World Bank Enterprise Surveys (*www.enterprisesurveys.org)*.
Creditor Rights (*CR*)	Index of regulations and procedures that affect the rights of creditors in the sample countries. Index for the cross section measures: This index, measure the degree to which collateral and bankruptcy laws facilitate lending. The index includes 3 aspects related to legal rights in bankruptcy and 7 aspects found in collateral law. A score of 1 is assigned for each of the following features of the law; 1) Secured creditors are able to seize their collateral when debtor enters reorganization – there is no automatic stay or asset freeze imposed by the court, 2) Secured creditors are paid first out of the proceeds from liquidating a bankrupt firm, 3) Management does not stay during reorganization, 4) a general, rather than specific, description of assets is permitted in collateral agreements, 5) General, rather than specific description of debt is permitted in collateral agreements, 6) Any legal or natural person may grant or take security in the property, 7) A unified registry that includes charges over movable property operates, 8) Secured creditors have priority outside of bankruptcy, 9) Parties may agree on enforcement procedures by contract, 10) Creditors may seize and sell collateral out of court. The index increases in strength of creditor rights, ranging from 0 to 10. Index for the panel measures: Whether 1) There are restrictions, such as creditor consent, when a debtor files for reorganization, 2) Secured creditors are able to seize their collateral after the petition for reorganization is approved (e.g. no automatic stay), 3) Secured creditors are paid first out of the proceeds of liquidating a bankrupt firm, and 4) Management does not stay during reorganization. A value of one is added to the index when a country's laws and regulations provide each of these powers to secured lenders. The creditors rights index aggregates the scores and varies between 0 (poor creditors' rights) and 4 (strong creditors rights). Source: Doing Business Report, available at *www.doingbusiness.org*. The 2002 index is from Djankov, McLiesh and Shleifer (2007).
Credit Information (*CI*)	A measure of the availability of credit information. Index for the cross section measures: This index measures rules affecting the scope, accessibility and quality of credit information available through either public or private bureaus. A score of 1 is assigned

	for each of the following 6 features of the credit information system; 1) Both positive and negative credit information is distributed, 2) Data on both firms and individuals are distributed, 3) Data from retailers, trade creditors or utilities as well as financial institutions are distributed, 4) More than 2 years of historical data are distributed, 5) Data on loans above 1% of income per capita are distributed, 6) By law, borrowers have the right to access their data. Index for the panel measures: An indicator for whether a public credit bureau or private credit registry operates in the country. The indicator takes on a value of 1 if either a public or private registry is in operation, 0 otherwise. Source: Doing Business Report (*www.doingbusiness.org*). The 2002 index is from Djankov, McLiesh and Shleifer (2007).
Court Time (*CourtTime*)	Measure of the time in calendar days to resolve a contract dispute through the court system in the sample countries. Higher values imply slower enforcement. Source: Doing Business Report (*www.doingbusiness.org*).
Court Enforcement (*Enforce*)	Measure of firms' perceptions regarding their confidence in the ability of the judiciary to enforce its decisions. Scale ranges from 1 to 6, with higher values associated with more confidence in the judiciary. *Enforce* is averaged at the regional level in every country. Source: World Bank Enterprise Surveys (*www.enterprisesurveys.org)*.
Court Speed (*CourtSpeed*)	Measure of firms' perceptions regarding their confidence in the ability of the judiciary to resolve business disputes quickly. Scale ranges from 1 to 6, with higher values associated with higher perception of judicial speed. *CourtSpeed* is averaged at the regional level in every country. Source: World Bank Enterprise Surveys (*www.enterprisesurveys.org)*.
Firm size (*Size*)	Size of firm, measured by number of permanent employees. Used as control variable for cross section analysis. Source: World Bank Enterprise Surveys (*www.enterprisesurveys.org)*.
Financial Statements Audited	Indicator of whether or not firms have had a formal audit of their financial statements. 1 indicates 'yes' and 2 indicates 'no'. Used as control variable for cross section analysis. Source: World Bank Enterprise Surveys (*www.enterprisesurveys.org)*.
Private Credit/GDP and *GNI*	Respectively, the ratio of credit to the private sector relative to GDP, and Gross National Income. Source: Djankov, McLiesh and Shleifer (2007).

Table 2: Summary Statistics

Country	Bank Investment Credit	Creditor Rights Index (Crosssection)	Creditor Right Index (Panel)	Credit Info Index (Crosssection)	Court Time (Days)	Court Enforce (0-6)	Court Speed (0-6)	Private Credit/GDP
Albania	18.16	9	3	0	390	3.53	2.50	9.9
	(31.45)					(1.46)	(1.20)	
Armenia	19.55	4	2	3	185	3.35	3.05	7.8
	(30.10)					(1.31)	(1.27)	
Bosnia	24.30	8	3	5	330	2.80	2.05	43.8
	(38.62)					(1.31)	(1.08)	
Bulgaria	17.35	6	2	3	440	3.59	1.86	37.1
	(32.76)					(1.71)	(1.17)	
Croatia	26.56	4	3	0	415	3.34	1.83	57.5
	(37.61)					(1.37)	(1.03)	
Czech	6.60	6	3	5	290	2.79	1.8	33.4
Republic	(21.67)					(1.21)	(1.02)	
Estonia	11.73	4		5	150	4.05	2.31	42.4
	(25.87)					(1.44)	(1.24)	
Georgia	23.22	7	2	0	375	3.78	2.54	9.7
	(37.84)					(1.48)	(1.33)	
Germany	18.49	8	3	6	175	4.10	3.15	112.3
	(29.22)					(1.18)	(1.33)	
Greece	12.04	3	1	4	151	4.66	2.69	78.6
	(25.77)					(1.47)	(1.29)	
Hungary	13.93	6	1	5	365	3.15	2.42	46.5
	(29.20)					(1.52)	(1.47)	
Ireland	25.54	8	1	5	217	3.46	2.10	136.9
	(39.24)					(1.53)	(1.10)	
Kazakhstan	13.69	5	2	0	380	3.41	2.62	28.3
	(28.06)					(1.45)	(1.31)	
Latvia	15.69	8	3	3	186	3.14	2.01	44.8
	(32.81)					(1.22)	(1.22)	
Lithuania	9.14	4	2	6	154	3.27	2.33	25.9
	(23.39)					(1.45)	(1.40)	
Macedonia	11.48	6	3	3	509	2.63	1.98	23.2
	(29.77)					(1.36)	(1.08)	
Moldova	15.88	6	2	0	340	2.60	2.29	21.3
	(29.92)					(1.31)	(1.21)	
Poland	9.34	3	1	4	980	2.98	2.02	27.7
	(22.86)					(1.27)	(1.10)	
Portugal	13.34	5	1	4	320	2.78	1.81	150.3
	(28.83)					(1.34)	(.981)	
Romania	13.65	4	1	4	335	3.41	2.52	10
	(28.84)					(1.42)	(1.32)	
Russia	4.20	3	2	0	330	3.31	2.12	24.5
	(18.58)					(1.48)	(1.23)	
Kyrgyzstan	12.86	8	3	2	492	3.38	2.36	7.09
	(27.85)					(1.60)	(1.34)	
Slovakia	9.46	9	2	2	565	3.56	2.09	31.2
	(25.84)					(1.42)	(1.10)	
Slovenia	16.25	6	3	3	913	3.59	1.93	46.3
	(29.53)					(1.50)	(0.92)	
Spain	16.73	5	2	6	169	3.75	2.18	125.4
	(33.04)					(1.44)	(1.18)	
Turkey	5.77	1	2	5	330	4.32	3.14	20.5
	(19.82)					(1.05)	(1.74)	
Ukraine	9.65	8	2	0	269	3.18	2.10	25
	(24.81)					(1.57)	(1.19)	

*Standard deviations across firms are shown in parentheses.
**All summary statistics shown are for cross-section data, year 2005.

Table 3: Changes in the Creditor Rights and Credit Information Indices Between 2002 and 2005

Country	Increase in Creditor Rights Index	Increase in Credit Information Index
Albania	0	0
Armenia	0	1
Azerbaijan	0	1
Bulgaria	1	0
Croatia	0	0
Czech Republic	0	1
Macedonia	0	0
Hungary	1	0
Kazakhstan	0	0
Kyrgyzstan	0	1
Latvia	0	1
Lithuania	0	0
Moldova	0	0
Poland	2	0
Romania	-2	0
Russia	1	0
Slovakia	0	0
Slovenia	0	0
Turkey	0	0
Ukraine	0	0
Yugoslavia	0	1

Notes:
1. The Creditor Rights (Panel) Index ranges from 0-4.
2. The Credit Information (Panel) Index is a dummy variable which is equal to one is there is a private credit bureau or a public credit registry.

Table 4: Cross-sectional Correlations

	Bank Investment Finance	CR	CI	Court Time	Court Enforce.	Court Speed
CR	0.0503*					
	(0.000)					
CI	0.0688*	-0.0657*				
	(0.000)	(0.0000)				
Court	-0.0503*	-0.2540*	-0.1649*			
Time	(0.000)	(0.000)	(0.000)			
Court	0.0069	0.0484*	0.1156*	-0.1485*		
Enforcement	(0.4762)	(0.000)	(0.000)	(0.000)		
Court	-0.0048	0.1032*	0.0911*	-0.1457*	0.4501*	
Speed	(0.6249)	(0.000)	(0.000)	(0.000)	(0.000)	
GNI	0.1175*	0.0792*	0.6403*	-0.3349*	0.1501*	0.0752*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)

Notes:
1. Significance level in parenthesis; * denotes 1% significance level.

Table 5: Correlations between Enforcement, National Income and Changes in CR and CI

	Δ CR	Δ CI	Court Time	Court Enforce.	Court Speed
Δ CI	-0.1282*				
	(0.000)				
Court	0.4195*	-0.2691*			
Time	(0.000)	(0.000)			
Court	-0.0786*	-0.1320*	-0.0309		
Enforcement	(0.006)	(0.000)	(0.279)		
Court	-0.2003*	0.2563*	-0.3465*	0.4960*	
Speed	(0.000)	(0.000)	(0.000)	(0.000)	
GNI	0.2588*	-0.5338*	0.4244*	0.1318*	-0.1815*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)

Notes:
1. ΔCR is the increase in CR between 2002 and 2005; Δ is the increase in CI between 2002 and 2005.

Table 6: Cross-Section Results on Bank Financing, Creditor Rights and Enforcement

DepVar	Bank Investment Finance					
	(1)	(2)	(3)	(4)	(5)	(6)
CR	1.804	1.581	.007	.094	.471	.492
	(.720)**	(.655)**	(.511)	(.495)	(.421)	(.397)
CI	.624	.233	.784	.350	.790	.348
	(.410)	(.581)	(.379)**	(.582)	(.375)**	(.577)
CourtTime	.007	.006				
	(.006)	(.006)				
CR*CourtTime	-.002	-.002				
	(.002)	(.002)				
Enforce			-1.537	-1.342		
			(.680)**	(.673)**		
Cr*Enforce			.305	.260		
			(.104)***	(.102)**		
CourtSpeed					-1.477	-1.176
					(.705)**	(.596)**
CR*CourtSpeed					.260	.221
					(.137)*	(.127)*
Size		.004		.004		.004
		(.001)***		(.001)***		(.001)***
log(GNI)		1.117		1.157		1.157
		(1.220)		(1.215)		(1.195)
Industry		-.848		-.840		-.851
		(.164)***		(.153)***		(.152)***
Ownership		2.436		2.387		2.424
		(.831)***		(.798)***		(.808)***
Obs.	9795	9795	9795	9795	9795	9795
R^2	.014	.025	.014	.026	.014	.025
F statistic	4.561	14.962	7.407	13.929	4.233	14.429

Notes:
1. Robust Standard Errors adjusted for clustering by country in parenthesis. *** denotes 1%, ** denotes 5%, and * denotes 10% level of significance.
2. Ownership is a dummy which equals one for single proprietor firms. Industry is a dummy equal to one for manufacturing firms, and zero for service firms.

Table 7: Cross-Section Results on Bank Loans, Creditor Rights and Enforcement

DepVar	Bank Loan					
	(1)	(2)	(3)	(4)	(5)	(6)
CR	.032	.022	.012	.015	.014	.014
	(.018)*	(.017)	(.013)	(.011)	(.009)	(.007)**
CI	.017	-.001	.017	-.001	.017	-.0005
	(.010)*	(.013)	(.009)*	(.014)	(.009)**	(.014)
CourtTime	-.00	-.00				
	(.0002)	(.0002)				
CR*CourtTime	.00	.00				
	(.00005)	(.00004)				
Enforce			-.029	-.021		
			(.017)*	(.018)		
CR*Enforce			.005	.004		
			(.003)**	(.003)		
CourtSpeed					-.048	-.036
					(.018)***	(.014)***
CR*CourtSpeed					.007	.005
					(.004)**	(.003)*
Size		.0001		.0001		.0001
		(.00003)***		(.00003)***		(.00003)***
log(GNI)		.049		.047		.045
		(.031)		(.030)		(.030)
Industry		-.027		-.026		-.026
		(.004)***		(.004)***		(.004)***
Ownership		.113		.113		.113
		(.030)***		(.029)***		(.029)***
Obs.	9795	9795	9795	9795	9795	9795
R^2	.023	.06	.024	.06	.026	.061
F statistic	7.24	15.08	12.31	16.35	10.71	15.58

Notes:
1. Robust Standard Errors adjusted for clustering by country in parenthesis. *** denotes 1%, ** denotes 5%, and * denotes 10% level of significance.
2. Ownership is a dummy which equals one for single proprietor firms. Industry is a dummy equal to one for manufacturing firms, and zero for service firms.

Table 8: Panel Regression Results on Creditor Rights and Court Time

DepVar:	Bank Investment Finance	Bank Investment Finance	Bank Loan	Bank Loan
	(1)	(2)	(3)	(4)
CR	4.306	6.145	10.791	7.655
	(1.803)**	(1.624)***	(1.964)***	(2.601)***
CR*CourtTime	-.007	-.013	-.024	-.014
	(.002)***	(.003)***	(.003)***	(.004)***
CI	-1.812	3.401	-6.269	-15.158
	(7.434)	(8.002)	(12.006)	(12.611)
CI*CourtTime	.001	-.011	.023	.046
	(.017)	(.018)	(.027)	(.028)
Time	4.754	-.459	5.987	14.876
	(1.899)**	(2.392)	(2.509)**	(4.59)***
Time*CourtTime		.013		-.021
		(.006)**		(.009)**
Firm FE	Y	Y	Y	Y
Obs.	2446	2446	2446	2446

Notes:
1. Robust Standard Errors adjusted for clustering by country in parenthesis. *** denotes 1% level, ** denotes 5% level, and * denotes 10% level of significance.
2. *CR*CourtTime* is the interaction of *CR* with *CourtTime*. *CI*CourtTime* is the interaction of *CI* with *CourtTime*.

Table 9: CR and Regional Averages of Firm-reported Judicial Efficiency

DepVar:	Bank Investment Finance	Bank Investment Finance	Bank Loan	Bank Loan
	(1)	(2)	(3)	(4)
CR	-22.089	-28.213	-56.882	-62.135
	(12.616)*	(12.882)**	(24.53)**	(22.665)***
CR*Enforce	6.698	8.516	16.574	18.133
	(3.934)*	(3.732)**	(7.934)**	(7.184)**
CI	-11.648	-29.139	22.126	7.123
	(15.428)	(17.646)*	(14.224)	(33.164)
CI*Enforce	3.343	8.594	-6.344	-1.84
	(5.399)	(5.902)	(3.99)	(10.332)
Time	4.392	21.883	4.862	19.865
	(1.988)**	(8.781)**	(2.993)	(30.105)
Time*Enforce		-5.25		-4.504
		(2.379)**		(9.53)
Firm FE	Y	Y	Y	Y
Obs.	2446	2446	2446	2446

Notes:
1. Robust Standard Errors adjusted for clustering by country in parenthesis. *** denotes 1% level, ** denotes 5% level, and * denotes 10% level of significance.
2. *CR*Enforce* is the interaction of *CR* with *Enforce*. *CI*Enforce* is the interaction of *CI* with *Enforce*.

Table 10: CR and Regional Averages of Firm-reported Judicial Speed

DepVar:	Bank Investment Finance	Bank Investment Finance	Bank Loan	Bank Loan
	(1)	(2)	(3)	(4)
CR	-23.163	-32.479	-44.092	-46.876
	(11.47)**	(9.625)***	(28.621)	(27.307)*
CR*CourtSpeed	9.917	13.595	17.721	18.82
	(4.827)**	(4.008)***	(11.693)	(11.029)*
CI	-6.041	-28.04	20.557	13.982
	(10.294)	(12.889)**	(14.116)	(26.303)
CI*CourtSpeed	1.693	11.151	-7.655	-4.828
	(4.79)	(5.839)*	(6.482)	(11.853)
Time	5.032	27.031	5.919	12.494
	(1.903)***	(7.981)***	(2.749)**	(22.36)
Time*CourtSpeed		-9.457		-2.826
		(3.335)***		(9.922)
Firm FE	Y	Y	Y	Y
Obs.	2446	2446	2446	2446

Notes:
1. Robust Standard Errors adjusted for clustering by country in parenthesis. *** denotes 1% level, ** denotes 5% level, and * denotes 10% level of significance.
2. *CR*CourtSpeed* is the interaction of *CR* with *CourtSpeed*. *CI*CourtSpeed* is the interaction of *CI* with *CourtSpeed*.

Table 11: CR, Enforcement and National Income

DepVar:	Bank Investment Finance	Bank Investment Finance	Bank Investment Finance
	(1)	(2)	(3)
CR	-1.313	-1.181	-1.919
	(3.173)	(3.237)	(3.186)
CI	-15.418	-11.556	-3.286
	(16.036)	(25.146)	(16.295)
CR*CourtTime	-.011		
	(.001)***		
CR*Enforce		6.072	
		(3.256)*	
CR*CourtSpeed			9.299
			(4.115)**
CR*log(GNI)	2.633	-1.050	-2.273
	(1.998)	(3.150)	(1.723)
Time	4.628	4.496	5.234
	(1.962)**	(2.063)**	(1.983)***
Firm FE	Y	Y	Y
Obs.	2446	2446	2446
R^2	.572	.571	.572

Notes:
1. Robust Standard Errors adjusted for clustering by country in parenthesis. *** denotes 1% level, ** denotes 5% level, and * denotes 10% level of significance.
2. *log(GNI)* is logarithm of gross national income (current U.S. Dollars), averaged over 2001-2003. *CR*log(GNI)* is the interaction of *CR* with *log(GNI)*.

Table 12: Time-varying Court Enforcement Measures

DepVar:	Bank Investment Finance		Bank Loan	
	(1)	(2)	(3)	(4)
CR	4.096	-19.727	8.743	-26.681
	(1.715)**	(10.039)**	(2.954)***	(27.136)
CI	1.896	-5.272	-8.389	22.021
	(4.899)	(13.179)	(9.045)	(30.682)
CourtTime	.001		.048	
	(.014)		(.027)*	
CR*CourtTime	-.008		-.021	
	(.002)***		(.004)***	
CI*CourtTime	-.011		.028	
	(.010)		(.018)	
Enforce		-17.069		-19.422
		(8.376)**		(20.034)
CR*Enforce		5.967		7.041
		(3.130)*		(8.154)
CI*Enforce		1.247		-6.364
		(4.240)		(9.195)
Time	4.263	4.518	5.908	4.807
	(1.837)**	(1.909)**	(2.205)***	(3.349)
Firm FE	Y	Y	Y	Y
Obs.	2446	2446	2446	2446

Notes:
1. Robust Standard Errors adjusted for clustering by country in parenthesis. *** denotes 1% level, ** denotes 5% level, and * denotes 10% level of significance.
2. *CR*CourtTime* (*CI*CourtTime*) is the interaction of *CR* (*CI*) with time-varying *CourtTime*.
3. *CR*Enforce*(*CI*Enforce*) is the interaction of *CR* (*CI*) with time-varying *Enforce*.

Table 13: CR, Court Time and Private Credit In 129 Countries During 1978-2003

DepVar:	PvtCredit/GDP	PvtCredit/GDP	PvtCredit/GDP
	(1)	(2)	(3)
CR	.076	.193	.221
	(.058)	(.077)**	(.077)***
CR*CourtTime	-.00002	-.0003	-.0004
	(.0001)	(.0002)**	(.0002)**
CourtTime	-.0003		
	(.0003)		
Year	.003	.007	.014
	(.002)**	(.002)***	(.003)***
Year*CourtTime			-1.00e-05
			(7.31e-06)***
Country FE		Y	Y
Obs.	2832	2832	2832
R^2	.148	.847	.855

Notes:
1. Robust Standard Errors adjusted for clustering by country in parenthesis. *** denotes 1% level, ** denotes 5% level, and * denotes 10% level of significance.
2. Data on 129 countries over 1978-2003 (Source: DMS). The dependent variable is ratio of Private Credit to GDP.

Table 14: Heterogenous Effects of Creditor Rights and Enforcement

Depvar:	Bank Investment Finance			
Firm Characteristic:	Firm Size		Firm Ownership	
	(1)	(2)	(3)	(4)
CR	3.401	1.501	4.840	7.042
	(2.015)*	(2.162)	(1.964)**	(1.752)***
CR*CourtTime	-.008	-.005	-.008	-.012
	(.002)***	(.003)**	(.002)***	(.002)***
CI	-1.799	-1.793	-1.898	-2.014
	(7.439)	(7.443)	(7.439)	(7.446)
CI*CourtTime	.001	.001	.001	.001
	(.017)	(.017)	(.017)	(.017)
Size*CR	.006	.021		
	(.006)	(.007)***		
Size*CR*CourtTime		-.002		
		(7.01e-04)***		
Govt*CR			.255	-4.056
			(1.753)	(2.672)
Govt*CR*CourtTime				.007
				(.003)**
Foreign*CR			-6.678	-25.180
			(7.266)	(9.319)***
Foreign*CR*CourtTime				.029
				(.010)***
Time	4.740	4.734	4.839	4.955
	(1.909)**	(1.913)**	(1.900)**	(1.901)***
Firm FE	Y	Y	Y	Y
Obs.	2446	2446	2446	2446

Notes:
1. Fixed Effects OLS panel estimations. Robust Standard Errors adjusted for clustering by country in parenthesis. *** denotes 1% level, ** denotes 5% level, and * denotes 10% level of significance.
2. *Size* is firm size in 2002, measured by the number of permanent workers in 2002.
3. *Govt.* is a dummy indicating majority government ownership, and *Foreign* a dummy indicating majority foreign ownership of the firm in 2002. Thus, the omitted ownership category is domestic private ownership.

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